LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

The Meaning of "Ionisation."

I AM sorry that Prof. Walker (p. 458) has avoided my question. At present I am not concerned either with his position or with mine, with van der Waals or with Newton—I wish simply to know *what exactly he would have us understand by the word ionisation.* I hold that it is our duty, as scientific workers, if possible, to be exact in word as well as deed. It is a matter of reproach to us that we should be lectured, year after year, from the chair of the Royal Society, for our carelessness as writers. Now that the attempt is being made to standardise all sorts of things—from amperes and ohms to the members of iron bridges, even by means of international congresses—we might well devote some attention to our words and attempt to standardise our scientific nomenclature. *Ionisation* is a word used with increasing frequency in these days—unfortunately also with increasing ambiguity. I would appeal to Prof. Walker, as a leader among British physical-chemists, at least to tell us what he wished us to understand when using the word recently—as his meaning is in no way made clear in his article.

HENRY E. ARMSTRONG.

The Flow of Sand.

ON Friday, February 11, I had the pleasure of hearing Mr. C. E. S. Phillips deliver the discourse at the Royal Institution, illustrated by many experiments, a number of which showed that when sand escapes from an orifice at the bottom of a long vertical tube it does not do so perfectly uniformly, but in a series of pulses which are sufficiently rapid to produce audible sounds. Mr. Phillips did not offer any suggestion as to the reason why the flow is regularly intermittent, but two of his other experiments, and the laws of friction, suggest a possible cause.

One experiment showed sand forming a cone on being poured from a funnel. The sloping sides of the cone gave the angle of repose, and it was noticed that the sand at first did not flow steadily down the slopes, but intermittently. This, I think, may be due to a combination of the momentum of the sliding sand and the difference between the statical and dynamical friction between the particles of the sand. A little heap of sand collects, then the statical friction is overcome, and the momentum carries the sand slightly too far, thus making the angle of repose too small; consequently the on-coming sand is able to remain stationary on the slope until in turn its angle of repose becomes too great, the statical friction is overcome, and the cycle is repeated. The other experiment showed how sand is self-supporting in a tube except for the cone of sand at the base. Allow this cone of sand to pass through the orifice, and the rest will fall intermittently in the manner indicated.

If this theory is correct, one would expect sand with a comparatively large coefficient of statical friction to give fewer pulses per second than a sand having a smaller angle of repose.

A. S. E. ACKERMANN.

25 Victoria Street, Westminster, London, S.W., February 14.

I AGREE with the explanation offered by Mr. Ackermann in the first part of his letter, for it is evident that sand must slip down itself by a series of rushes.

The process, however, by which a mass of sand falling in a glass tube produces musical sounds is somewhat more complicated. The column must be regarded as consisting of two parts, the upper portion acting simply as an intermittently moving piston. It is the central region of the lower part which becomes less dense, owing to escape of sand through the orifice; the upper portion, being no longer supported, slips downward as a whole.

The rapidity of its intermittent motion depends upon the friction between the glass and sand. Hence the pitch of the note is raised if the grains are better packed. The action appears to resemble that of pushing a moist finger-tip along a polished table. The finger jumps rapidly and regularly.

As soon as the column so far lowers that the previously compact upper portion begins to fall away at its centre, all sound ceases. I showed at the Royal Institution that by coating the inner surface of the glass tube with oil, before filling it with sand, the column moved downward by slow, regular jerks, increasing in rapidity as the mass of the remaining sand in the tube grew less. Here all friction between the glass and the sand grains was eliminated, on account of the outer layer of particles adhering to the oil and remaining as a coating upon the tube.

The jerks became more rapid as the inertia diminished with the decreasing mass, which also explains why the pitch of the note given out by a tube rises somewhat as the sand column diminishes.

CHARLES E. S. PHILLIPS.

Castle House, Shooters Hill, Kent, February 15.

The Heredity of Sex.

CURRENT Mendelian theories of the heredity of sex are based on the assumption that gametes are pure with respect to sex characters; that is, that a gamete may carry the factor for maleness or the factor for femaleness, but not both. This view may be expressed thus:—a gamete carries M, the factor (or factors) for maleness, or F, the factor (or factors) for femaleness, but not both M and F.

The hypothesis proposed in this note suggests that the phenomena of sex are due, not to a single pair of allelomorphic characters, but to two independent pairs of characters, namely, maleness (M), with its allelomorph, absence of maleness (*m*), which constitute one pair, and femaleness (F), with its allelomorph (*f*), which constitute the other pair. On this hypothesis, since M*m*, F*f* are independent of one another, representatives of both pairs of characters occur in every gamete.

All gametes are therefore of one or other of the following sex constitutions, MF, M*f*, *m*F, *mf*. Hence all zygotes produced by the pairing of such gametes are of one or other of the following nine gametic constitutions:—

Dihybrid scheme:

1 MMFF 2 MMF*f* 2 M*m*FF 4 M*m*F*f*	9 MF
1 MM*ff* 2 M*mff*	3 M*f*
1 *mm*FF 2 *mm*F*f*	3 *m*F
1 *mmff*	1 *mf*

In zygotes MMFF and M*m*F*f* it may be predicted that circumstances, nutrition, &c., determine which type (male or female) of sexual organs is produced.

Thus double begonias, which bear female flowers, may be induced by starvation to bear male flowers. Fern prothalli, which bear normally male and female organs, produce, when subjected to special treatment, male organs only, and so on.

In general, the numbers of "males" and "females" among MMFF and M*m*F*f*, zygotes, will be about equal, though wide departures from equality may occur in any species owing to the prevalence of conditions which favour the production of male or female organs.

The following types of zygotes will, it may be supposed, produce male sexual organs, MMF*f*, MM*ff*, M*mff*, and the following, female organs, M*m*FF, *mm*FF, *mm*F*f*; hence the number of males will equal the number of females produced by such zygotes.

The *mmff*, pure recessive type of zygote, if viable, is sterile. The origin of sexuality connotes an origin of sterility. To give examples of the application of the hypothesis to biological facts:—

A zygote of the MF type produces gametes of which all or some carry MF. Species which have MF gametes will be capable of exhibiting parthenogenesis (natural or induced). Certain of the lower algæ produce "gametes" which may fuse in pairs to form zygotes, or may develop directly into new individuals. Those which behave in the former fashion may be such as carry M*f* or *m*F, and those which develop directly may be the MF gametes.

Among moulds, certain species of Mucor exist as several races the individuals of each of which reproduce themselves asexually, but do not conjugate with one another. When, however, individuals of different races meet, they conjugate and produce zygospores. It may be supposed that one race is of such a type as MM*ff*, another of the *mm*FF type. In this case verification of the hypothesis is possible.

The absence of sexual reproduction in various groups of fungi is to be explained on the present hypothesis as due to the extinction (or effective separation) of all zygotes except those of one type, *e.g.* the M*f* or the *m*F types.

In homosporous ferns, the spores, produced after the reduction division, give rise each to a prothallus which bears male and female organs. If it be allowed that the reduction division is of fundamental significance with respect to the segregation of characters, it would appear to follow that current Mendelian theories of sex-heredity fail to account for the fact that a spore produced as a consequence of the reduction division may yet carry "male" and "female" factors.

The phenomena may be interpreted simply in terms of the new hypothesis. The fern plant is MMFF; the spore, and hence the prothallus, carries MF. Therefore male and female organs may be produced by the prothallus. The gametes formed and matured in the female organs are "female," those formed and matured in the male organs are "male."

In the heterosporous ferns the spores are of two kinds, macrospores, giving rise to "female" prothalli, and microspores, which give rise to "male" prothalli. In terms of our hypothesis the sporophyte (zygote) is M*m*F*f*, the megaspore *m*F, and the microspore M*f*.

Further, the high rate of mortality which accompanies spore-formation receives on this hypothesis an intelligible explanation. It is due to the inevitable reappearance of combinations of sex-characters which the heterosporous fern has ceased to tolerate.

In the light of the present hypothesis, homosporous ferns are homosporous because they are homozygous, and heterosporous ferns are heterosporous because they are heterozygous for the sex characters M and F.

The significant question arises, How far is the present limitation of characters presented by any great group of organisms determined by the fact that in this group the task of reproduction has come to be committed to some particular type or types of gametes?

The hypothesis would appear to throw light on large numbers of known facts, on prepotency, partial sterility—such, for example, as occurs in heterostylism—the apparently excessive production of pollen and ovules, and so forth.

Not only is it not repugnant to a reasonable explanation of many facts, but also the hypothesis does not seem to be inherently improbable. In that it is based on the presence and absence theory, it receives the sanction of Mendelism. It tempts the imagination to trace the origin of sexuality from the "self-contained" organisms of the MF type. Evolution in such types took, in some individuals, the form of a dropping out of the M, in others, of a dropping out of the F, factor. Such incomplete forms as M*f* and F*m* discovered in fusion the means of restoring their constitutions; but out of this fusion possibilities for novel constitutions arose, for the M*m*F*f* type of zygote was now in being. In reproducing by segregation the original MF type of gamete, the zygote was constrained to produce likewise the other possible combinations of M*m* and F*f*. Fusions between the several types resulted in different forms of zygote; evolution had its chance.

Among other types, the pure recessive, *mmff*, arose, and, with its advent, sterility, and, it may be, death, came on the scene as the sinister shadow of "sexual" reproduction.

It only remains to add to this note that—in case the hypothesis it proposes prove of value—though the responsibility for the hypothesis rests with the writer, the stimulus to which it owes its inception originated, in the first place, from a study of Bateson's work on heredity, and in the second place from discussions on the problems of heredity between the writer and his colleagues, Miss Rayner, Mr. Jones, and Miss Pellew, of the botanical laboratory, to whom certain of the foregoing illustrations are due.

University College, Reading. FREDERICK KEEBLE.

Geology and the Earth's Axis of Rotation.

FROM time to time the pages of NATURE contain references to the theory which would explain the occurrence of Ice ages by a hypothetical shifting of the earth's axis of rotation. On the face of it, the theory in question appears to be capable of explaining a good deal more than this.

In the first place, if the axis of rotation were to be shifted, it seems clear that the relations between the earth's hydrosphere (or hydrospheroid) and the lithosphere must undergo change. In the regions towards which the pole is approaching land will tend to emerge from the sea, and *vice versa*. If the effects of this supposition be traced out in detail, they will be found to furnish an explanation of such phenomena as raised beaches, submerged river valleys, varying continental connections, &c., without postulating violent alterations in the lithosphere itself. Speaking merely qualitatively, the hypothesis seems to fit the facts pretty closely, *e.g.* (*a*) the height of raised beaches tends to increase as one approaches the polar regions, as it ought; (*b*) a marine transgression is associated with a warm climate.

In the second place, a shifting of the polar axis will not be without effect on the lithosphere itself, although such effect would not, presumably, under present conditions, at all resemble the effect on the hydrosphere already alluded to. Even in a rough qualitative way this effect is not easily traced out, but it seems tolerably clear that it will account for those processes of folding, &c., whereby mountain chains are built up, and also for extensive local subsidences such as are believed to have occurred in geological time. These, and doubtless other phenomena, the hypothesis explains without having recourse to the supposition that the earth has been undergoing contraction through loss of heat.

I am not aware of the existence of any publications dealing with the matters referred to, but as the subject appears to be not without interest, perhaps some of your other readers will be able to refer me to papers, &c., treating of the subject with which they may be acquainted. I should be particularly glad to be referred to researches in which the subject is treated quantitatively.

HUGH BIRRELL.

Holyrood House, Bo'ness, Linlithgowshire, N.B.,
February 4.

Secondary Cells in Tropical Climates.

ALL who have used batteries of small secondary cells in the tropics will have experienced the difficulty of keeping their cells in efficient working order, and especially in preserving the junction of separate cells from rapid corrosion. The difficulty, appreciable in Europe, becomes very serious in a climate where the laboratory temperature lies between 30° and 40° C., and for this reason—it is probable that practically all accumulators sent to tropical countries by European manufacturers are filled by their recipients with dilute sulphuric acid of a density (1,190) which corresponds to a 20 per cent. mixture in north Europe at 15° to 20° C., but at a temperature of 30° to 35° C. indicates a mixture which is far too rich in acid for the health of the cells. Some simple experiments recently carried out in this laboratory exhibit quite clearly how large a deviation from the standard 20 per cent. mixture is caused by filling cells at 30° with dilute acid of density 1,190. It is found that a density of 1,190 at 30° corresponds to a composition of 23 per cent., whereas the value of the composition accepted as giving the best results with cells of this type is 20 per cent. The difference is as much as half the total change in composition due to chemical action during the process of charging the cell.

The conclusion reached from an examination of the density-temperature curves for dilute sulphuric acid points to the advisability of filling all secondary cells in localities where the average temperature is 30° or more with acid solution of density about 1.170. Densities as low even as 1,150 have been found satisfactory for small secondary cells in the hot weather in Calcutta.

In the case of large plants in power stations, the matter may be still more important, as a cell containing too strong